EGG-LAYING OF DEIDAMIA INSCRIPTA.

On June 4, 1897, I found on the under side of a leaf of *Ampelopsis quinquefolia*, seven eggs irregularly set, but evidently laid at the same date, and that a recent one. The eggs were very dark green and, under a glass, showed slight "facetting," and were unmistakably sphingid. Further search brought to light a second leaf with six eggs, and a tendril with two.

Four days later these eggs all hatched, giving larvae marked with a reddish spot behind and below the caudal horn. The larvae were yellow with almost no tinge of green, and remained so after feeding for three days. My previous experience with young sphingid larvae has been that they grew green after one day's feeding. The caudal horn was black with the tip white and ending in two setae.

On June 17 I found, on the same vine, four similar eggs, but laid among the flower-buds in such wise as to be well concealed by their strong resemblance to the buds in both size and color. As the buds grew lighter in color before opening, the eggs grew lighter by the yellower tint of the developing larvae, so that they still were like the buds! It is one of the most perfect bits of protective imitation I have seen, and fully explains why I had before found no eggs, but only young larvae, of *D. inscripta* — as these proved.

After the first eggs found among the buds I hunted every bud-cluster within easy reach and found twenty more!

The first brood of larvae moulted but three times, and fed but twenty days, the second being the moult omitted. In spite of this the larvae grew to full size. The second brood has moulted three times at the usual intervals, and will probably moult again in three days.

Caroline G. Soule.

Brookline, July 1, 1897.

A REPLY TO THE CRITIC OF PSYCHE.

The critic of Psyche is kind enough to notice "the Butterflies of Hildesheim" and concludes his remarks with the following paragraph:

"The scheme is based solely on the wing-neuration and has its merits and demerits on this ground. The most striking innovation is the primary subdivision which ignores previous dichotomy by leaving the Hesperiidae in conjunction with others; a minor one is the separation with family signification of Nemeobius from the Riodinidae; it shows the length to which one may go in discussing classification from a single standpoint."

Perhaps the shortest and most complete reply to the above is, that had I discussed the classification of Nemeobius from a single standpoint and that standpoint the neuration, I should have referred the genus to the Pieridae. That I did not do so, that everywhere I have pointed out the characters of convergence in the neuration, that my study of the latter is an attempt to show, however imperfectly and for the first time, the *direction* which the evolution of the veining assumes with the butterflies and that this direction is held and the characters repeated in distinct groups — all this seems to have been overlooked by the critic. With regard to Nemeobius I show that, while the Riodinidae are hardly separable from the Lycaenidae (Zephyrini) on pterogostic grounds, the neuration of *Nemeobius lucina* contradicts the same characters in both the Riodinidae and Lycaenidae. To unite it with either of these groups is to do violence to characters which have been long in forming, whereas to divide the Riodinidae from the Lycaenidae is to lay stress, perhaps undue stress, upon characters which have manifestly taken a shorter period to bring out. By a parity of reasoning I must conclude that the "family" importance of Nemeobius must be granted.